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Beyond Classic Scribes and Others' Dia-tribes: Ethnography and History along the New Guinea South Coast

I owe genuine thanks to Gilbert Herdt, Pierre Lemonnier, and Andrew Strathern for providing a context for constructive dialogue. This is particularly the case insofar as other recent invectives threaten to make the productive discussion of Pacific ethnography and history into something of an endangered species.[1] That each present reviewer comes to my book with a different set of scholarly interests provides a welcome opportunity for the ethnography of south coast New Guinea to refract onto disparate theoretical and topical agendas. As a prelude to their concerns, I add others they did not mention in order to broaden the purview of ensuing discussion.

In the context of 1990s anthropology, it may be asked why *South Coast New Guinea Cultures (SCNGC)* does not deal more explicitly with late colonial and postcolonial developments. What is the larger value and context of a work that considers a culture area--albeit a particularly rich and diverse one--on the basis of precolonial customs and those persisting into the very early colonial period? The effect of colonial impact and Southeast Asian trading influences is arguably underplayed in the volume's analysis of the New Guinea south coast. What would be the benefit (and the cost) of foregrounding these influences in the books analysis? Relatedly, how do such criticisms relate to Herdt's concern about how indigenous voices are objectified in the history of Melanesian ethnography and re-presented in the present? More generally, in what ways does the book speak to current directions in the cultural anthropology of Melanesia and to Melanesians' own concerns?

The historiographic dimension is a prominent part of the book's argument and is considered by all three reviewers. Though they all look favorably upon the volume's general orientation and depictions, my analysis has a different status for each commentator. Starting from their specific concerns, my response weaves discussion into a gradually widening arc that ultimately addresses the additional issues raised above.

First, I am most pleased that the scholarship and substantive portrayals of *SCNGC* have found such a warm and generally approving reception. It is important to know that detailed ethnographic and comparative analysis is still valued in cultural anthropology. As against myself, I would only caution again that my use of untranslated German and especially Dutch sources was minimal, and that the available literature places significant limits on what

one can glean about more recent south coast developments, on the one hand, and their intricacies of subjective experience, on the other.

Types and Tensions: Great-Men, Big-Men, and Other-Men

Since the tensions and difficulties of a comparative analysis are often as interesting as its accepted contributions, problematics can be considered straightaway. As against the books main themes, Strathern and Lemonnier give special attention to my discussion of great-man versus big-man typologies and to my argument that it is quite difficult to apply these types effectively along the New Guinea south coast. Strathern finds the refractoriness of south coast political organization to the model of "great-man" versus "big-man" to be welcome. This conclusion resonates with counterindications against the original big-man model that Strathern himself documented among the Melpa of the New Guinea highlands some twenty-five years ago (1971). He has recently elaborated complementary points in a southern highlands context (1993a). *SCNGC* documents that south coast New Guinea leadership has been configured through diverse constellations of ritual, socioeconomic, and military control; these are difficult to reduce to received leadership and exchange types. But Strathern still finds it distressing that the critique of leadership types frequently references such works as the 1991 volume *Big Men and Great Men,* edited by Godelier and M. Strathern.

By contrast, though Lemonnier also agrees with me that big-man and great-man characteristics were confounded along the south coast, he suggests that this evidence should be used to refine rather than reject the big-man/great-man model. Hence, what Strathern sees in my book as nails in the coffin of a previously surpassed typology, Lemonnier sees as grist for extending the model--it becomes a future hope rather than a historical relic.

My own position lies between these two. Lemonnier's assessment to the contrary, it is not my intention to dismiss the big-man/great-man model in any general sense. What I do emphasize is that it is an ideal type; its value is heuristic and variable depending on the region and the cases it is applied to. This implicates the scale of analysis: Is the typology used roughly across Melanesia as a whole, or does it intend to explain intricate variations within a particular region? In my own opinion, the big-man/great-man model has been valuable in stimulating the comparative assessment of Melanesian leadership types beyond the original model of Melanesian big-men (Sahlins 1963). Though Sahlins's big-man archetype was quickly questioned by Strathern and others on ethnographic grounds, it continued to be influential in the face of such evidence. Like Strathern's own distinction between lead-

ership based on finance versus that based on production (1969, 1978), the big-man/great-man model has heuristic value in giving us at least two types of nonrank leadership to choose from, rather than one. However, the model is far less useful for understanding ranges of variation internal to particular culture areas of Melanesia; these variants often make mincemeat of its distinctions. In particular (and here I side more with Strathern--especially Strathern 1993a), it is important to emphasize dimensions of leadership that articulate more closely to cultural particularities. In New Guinea, these have included assumptions of ritual and spiritual power that often underlie political and economic structures of exchange and prestige. The ability of symbolic formulations to exert a genuine and nonreducible impact upon leadership has been effectively emphasized by Lindstrom (1984), as well as in my own earlier work (Knauft 1985a, 1985b) and that of other Melanesianists. Cultural orientations exert both local and regional influence.

On a broad, panregional level, the reification of abstract and less culturally sensitive distinctions may continue to have heuristic value. Such distinctions frequently link (with greater or lesser explicitness) to the kind of global contrasts that run through many of anthropology's ancestral legacies, including Lévi-Strauss's contrast between complex and restrictive structures of exchange; Durkheim's division between organic and mechanical solidarity; and Weber's distinction between this-worldly and other-worldly asceticism. The heritage of these global contrasts continues to inflect current typologies. In the present case, the big-man type entails more complex exchange based on economic calculation, whereas the great-man category includes restricted exchange and sanctified ritualism.

I find nothing wrong with large-scale typologies--as long as they are used at an appropriately large scale of analysis and recognized as the ideal types they in fact are. In a different context, I have myself developed a model of large-scale contrasts that is more encompassing even than those presently discussed: archetypes of sociality and violence across the transition from hominoid to simple human to more complex pre-state societies (Knauft 1989b, 1991, 1993, 1994a, 1994c, n.d.*b*). But it is important for all such models to remain open to refutation at refined levels of analysis--and not just to subdivide into a greater number of static types in the face of such evidence. As I noted in *SCNGC* (p. 125), models such as Strathern's (1969), which accommodate a sliding scale of empirical variation, are more responsive to local variants than those that do not, such as the big-man/great-man model (see also Lederman 1990).

SCNGC brings to light key features of south coast fertility cosmology; worldviews in this region were remarkable if not unique in emphasizing the creation of fertility through ritual sexuality and the taking of life-force

through head-hunting. These complementary processes form a distinct permutation upon cultural cycles of growth and depletion found elsewhere in Melanesia (Knauft 1989b; M. Strathern 1988). Moreover, internal variants of this shared emphasis along some two thousand kilometers of coastline provide the key point of entry for explaining enormous local differences in political, economic, and gendered features that were evident within the south coast region. As *SCNGC* shows, it is extremely difficult to account for these variations without taking the region's cultural orientations into account.

One of the important goals of *SCNGC* was to articulate rather than divorce sociopolitical features and symbolic orientations. Though I agree with Lemonnier that social relations and institutions provide a necessary basis of analysis, I disagree that they are sufficient engines of explanation when disarticulated from cultural dispositions. Subjective motivations not only make social life meaningful to actors but are integral to explaining empirical variation in their practices over space and time.

In this respect, I find my own model more empirically responsive than Lemonnier's. Defending discrete exchange logics in cases where virtually all their features are mixed together turns each instance into a complicated "intermediate case" that needs special explanation. In the process, the model's 'assumptions become virtually immune to ethnographic refutation. Lemonnier writes, "[I]t is less the *presence* of a given aspect of social organization that is important than its *possible* involvement in social relations and practices" (emphasis altered). As I stated in *SCNGC* (p. 83), "If they do not have some systematic empirical reflection . . . the existence of underlying exchange logics at the level of 'deep structure' is correspondingly called into question."[2]

Amid the confusion of exchange archetypes, Lemonnier takes the relative absence of pigs in exchange along the south coast as a key factor. Excepting the Elema, I concur wholeheartedly that this contrast is significant from a pan-Melanesian perspective. But when considering the *internal* complexities of south coast New Guinea as a region, using intergroup pig exchange as a diagnostic feature has the effect of defining the region by what it lacks; it does little to appreciate the richness and diversity of this important part of Melanesia in its own right. The absence of large-scale pig raising along the south coast is largely a function of raw environmental constraint rather than of cultural disinterest per se. As I tried to document, what is indeed remarkable for the south coast is how complex and developed the political economy of alliance and exchange was *even in the absence of large-scale transactions of pigs.* Correspondingly, I disagree with Lemonnier's assessment that the south coast has a "systematic absence of relation-

ship between various spheres of exchange." The point, rather, is that this relationship is a preeminently cultural one based on fertility cycles--the exchange of sexual fluids and life-force--rather than one based on a restricted notion of exchange based on pigs. Viewed intraregionally, what is needed is not analysis based on pig-absence but one that articulates culturally constituted notions of fertility and exchange with the socioecological potentials and constraints of their actualization. That this system enabled hereditary chiefdomship among the Purari is particularly telling; ranked political leadership was present along this section of the New Guinea south coast but absent in the New Guinea highlands, despite subsistence intensification and large-scale pig husbandry in the latter area (e.g., Strathern 1987).

At the level of ethnographic specifics, I can appreciate some of Lemonnier concerns, but the facts short-circuit his suspicions. What he characterizes as "short sharp formulae" describing south coast areas are only shorthand glosses for a host of ethnographically documented patterns. (The same goes for "fertility" as an overall concept; it is less an imposed cover-term than a gloss on the beliefs and customs that south New Guineans themselves found important.) The Trans-Fly fighting ethic that Lemonnier would like to stress was certainly not absent, but it was far weaker and more ineffectual than among neighboring groups. In opening his chapter on Keraki warfare, Williams emphasizes,

> It may be that more stalwart generations in the past possessed a larger share of the combative spirit, or merely a stronger taste for blood; but this, if it were ever the case, must have been long ago, for the oldest surviving witnesses can now recall only a few martial experiences. It would be wrong, therefore, to think that head-hunting among the Keraki was the absorbing aim which it seems to have been among the Marind or the virile population to the north of the Morehead district. (1936:262)

Correspondingly, Williams's chapter on Keraki leadership reveals that prestige was more a function of age, speech making, and gardening prowess than exploits in war (ibid.:ch. 13). Concerning the nominal ideology of "head challenges" that Lemonnier cites in cases of Trans-Fly adultery, Williams suggests that such challenges were rhetorical (ibid.:286); they were never, to his knowledge, ever taken up!--"I have no evidence that this . . . course was ever taken, and it is perhaps unlikely that an expedition would be organized for such a trivial cause." Indeed, "I have not been able to discover any ceremonial necessity for head-hunting among the Keraki, nor any kind of permanent obligation such as would lead to recurrent raiding. The very

infrequency of raids, which I have already stressed, indicates the absence of such an obligation" (ibid.:284). As Lemonnier himself notes in a different context, "Keraki power is directly and *primarily* linked to a man's skill as a gardener and organizer of feasts" (1993a:138; Lemonnier's emphasis).

Lemonnier's doubts about the timing and magnitude of Kiwai feasts also appear ungrounded. Landtman notes that major sections of the entire Kiwai Island population aggregated at the settlement holding the ceremony, where they were hosted in huge longhouses up to 450 feet in length (1927:204). Haddon notes that ritual season visits often lasted up to two or three months in duration (1901:97). Indeed, the missionary James Chalmers dreaded the ceremonial season because so much of the Kiwai population departed from their normal villages for such a long period of time. As for the timing of celebrations, the ceremony of competitive gift giving *(gaera)* occurred when gardens were harvested after the conclusion of the wet season, that is, in April (Landtman 1927:383). This is the same period mentioned for the ancestral pantomime or *horiomu* ceremony, which occurred after the spirits had fasted for the whole of the wet season (ibid.:333). In addition, the *mimia* or fire ceremony occurred directly prior to this, in March (p. 368). Given articulation between various ceremonial activities, the need for seasonally abundant foodstuffs to host large-scale population aggregations, and the known existence of a prolonged ritual season, the ceremonial aggregation and subsequent dispersal of Kiwai seems more than well attested.

As to Lemonnier's query about whether the Purari were really less avid in head-hunting than some other south coast groups, the distinctive fact is that, as Williams stresses, "the [Purari] raiders were usually content with a single victim" (1924:108). Raiding expeditions were for the most part undertaken only for infrequent ceremonial occasions such as the consecration of a new longhouse or the construction of an associated spirit effigy *(kaiemunu).* Given the large population of Purari villages, this low casualty rate stands in diametric contrast to the Asmat, Marind, and Kiwai, among whom many heads were taken on raids despite their smaller settlement sizes.

Lemonnier's suggestion that theoretical perspectives such as postmodem feminism are irrelevant to the appreciation of sexual pluralities in south New Guinea seemed to have missed the issue. My point here is that the recent tendency to typecast south New Guinea in comparative sexual terms was *not* transcended by many anthropologists *until they were pushed to do so* by the kinds of pluralizing awareness illustrated by Foucauldian and post-Foucauldian feminist analysis. These encourage a more diverse and non-essentialized perspective on sexual practices. To say that theoretical developments can sometimes be "only academic" is itself part of the larger point I was myself trying to make in a more nuanced historical way.

Stepping back from such transient disagreements, I am cognizant of Lemonnier's sensitive and comprehensive attempt to configure not just the New Guinea south coast but much of the island's sociopolitical complexity as variants upon the big-man/great-man model. I find this to be an ambitious and important enterprise. Like all such enterprises, it should benefit from disconfirmations as well as from cases it can more easily subsume, as Lemonnier himself notes both here and in his other writings. It may be underscored that these complexities are more fully spelled out in the French than in the English versions of his corpus on this issue (e.g., Lemonnier 1990, 1993b; cf. 1991, 1993a). Lemonnier himself has noted that ceremonial exchange and other features in south coast New Guinea "set these societies apart" from both big-man and great-man categories (1993a:139). I am impressed with the detail of his associated attempt to consider leaders from this region as interstitial between these types (ibid.). Though we might disagree about which features to privilege in making such distinctions, I applaud Lemonnier's dedication to ethnographic sophistication in the context of regional comparison.

Histories and Subjectivities

It is at this point that larger questions of evolutionary and historical progression arise, as mentioned most directly by Strathern. If the previous use of the big-man label is now realized to have been overgeneralized, this is not to deny real patterns of sociopolitical intensification in the core New Guinea highlands. My point about "chimeric" big-men in *SCNGC* (p. 120) pertained quite explicitly to recent changes of conceptual emphasis *among Melanesianists themselves* and *not* to the presence or absence of ethnographic circumstances (regardless what one calls them). I would be the last to deny the importance or the complexity of socioeconomic intensification and political alliance based on what has often been termed "bigmanship" in core areas of the New Guinea highlands.

However, it may now be shortsighted to repackage the Melanesian big-man or his alter ego without considering our attempts to understand postcolonial changes in Melanesian leadership. Rather than either dismissing the notion of political leadership types or relegating them to a past history, we can broaden our understanding and bring it more up to date (e.g., Strathern 1984, 1993b). Crosscutting the legacy of the big-man and the great-man is now the *raskol*-man, the *bisnis*-man or "develop-man," the *kastom*-man, the *lotu*-man or church-man, and the parliamentary-man, office-man or "*gav*-man." We need new models of culture as well as of exchange, politics, and institutional organization to comprehend these emergent types, and, more

importantly, the dynamics that interconnect them. Historical change should be no more an impediment to such conceptualizations than it was to anthropologists of the 1950s and 60s who found that Australian pacification and Western importation of pearl shells vastly increased (not "created") the exchange-based networks of those described as "big-men."

On the other hand, models that focus on highland or interior areas of New Guinea are often not as well suited to lowland areas such as the south coast. One of the larger purposes of *SCNGC* was to highlight a region of Melanesia that has been ethnographically remarkable but ethnologically backgrounded by the dominant focus on New Guinea highland societies. It is noteworthy that the criticisms considered above entrain a response that orients as if by necessity back toward theoretical models geared in the first instance to highland New Guinea. These were only a secondary concern in *SCNGC* itself.

It is at this juncture that Gilbert Herdt's insistence on greater detail in local patterns of subjectivity is a necessary and important corrective. Though I tried to push this type of analysis as far as I felt comfortable (and probably farther than either Lemonnier or Strathern might have wished), I admit frustration in not finding information that would have enabled a more fine-grained treatment. (Even tallies indicating the populations practicing ritual homosexuality are crude: "One cannot assume that these figures give more than a rough estimate" [*SCNGC,* p. 48].) Certainly, local beliefs, idioms, and subjectivities merit closer attention, which is what Herdt gently wishes in my discussion. As is the case with historical change and details of political economy, however, the ethnographic record for south coast ethnopsychology is limited. The principal accounts remain rich on other topics, especially when judged against ethnographic standards otherwise prevalent during the 1910s, 20s, and 30s. But the ethnography of subjective states and meanings, much less of fantasy and personal accounts of sexual experience, is largely absent. The kind of work that Herdt has himself inaugurated both in New Guinea and in the United States over the past fifteen years has often been path breaking in just this respect (e.g., 1981, 1982, 1984, 1987, 1992, 1994; Herdt and Stoller 1990; Herdt and Boxer 1993). I have resonated with this emphasis in parts of my corpus on the Gebusi of interior south New Guinea, who lie outside the south coastal purview (e.g., Knauft 1985a, 1985b, 1986, 1987b, 1989b, 1990). I have also directly analyzed the comparative psychodynamics of Melanesian homosexuality in a separate article (1987a). Since the publication of *SCNGC,* I have more recently extended and refined the discussion of subjectivity, sexuality, and self-constitution along the south coast (Knauft n.d.*a*:ch. 5; 1994a:412ff.). In short, the issues Herdt raises are ones I have found important. But given the accounts available and my desire

to emphasize those features most verifiable, I found it difficult to adequately explore issues of subjectivation and its relation to sexuality in *SCNGC* itself.

Contemporary Developments and "New Melanesian Anthropology"

The concerns of Herdt and Strathern, as well as Lemonnier, point to further stones left unturned, including those raised in my introduction above. How do subjective ontologies, diverse voices, and gendered or sexualized practices relate to changing socioeconomic and political conditions in the postcolonial present? Of what value is a largely historical ethnographic study such as *SCNGC?* The first point to note is that recent developments are eminently studiable and worthy of study in places such as the New Guinea south coast (especially on the PNG side, for which research permits are more readily obtainable). Even as some selected areas of New Guinea receive overwhelming attention, others, such as the south coast, are woefully neglected. Almost all the primary monographs on south coast New Guinea societies are based on field research that is now several decades old. To my knowledge, no major monographs taking colonial or postcolonial developments squarely into account have been published on any of the areas highlighted in *SCNGC,* excepting only Robert Maher's book on the Purari, which appeared thirty-four years ago (1961).[3] Given the paucity of literature on current developments, works such as *SCNGC* serve the purpose of restimulating interest in previous work, recasting its current theoretical relevance, and providing a baseline against which a new generation of research might advance. With this in mind, the present book exposes the way that ethnography's theoretic and ethnographic genealogy has been woven, so we can rediscover its strengths without uncritically thinking that our current interests exist in a historical vacuum.

So what of colonial and postcolonial change? One good criticism, which I owe to the cogent remarks of Stuart Kirsch and James Weiner (pers. corns.), among others, is that *SCNGC* underplays early economic intrusions, including the trade in bird-of-paradise feathers that affected south coastal areas of Dutch New Guinea during the 1920s and 30s. During ensuing decades, the south coast has been strongly affected by missionization, selected economic development schemes, and social and religious movements, including some that used to go under the name of "cargo cults" (*SCNGC,* p. 224). Relative to other coastlines of Oceania, the south coast of New Guinea remained an economic backwater during the early decades of this century; in contrast to adjacent coastal regions, colonial influence was quite spotty and sporadic (ch. 2). Correspondingly, the early ethnography of indigenous south coastal

beliefs and customs is exceptionally rich and worthy of attention. But notwithstanding these trends, a detailed inspection of patrol reports, mission records, and other manuscripts now lodged in Australia, the Netherlands, and Papua New Guinea would reveal greater outside impact than my account might suggest. Though early colonial contact may have been relatively late in comparative world-historical terms, it is important to reemphasize that pacification and suppression of indigenous fertility cults and sexual practices had dramatic if not catastrophic effects on local cultures along the south New Guinea coast during the first half of the twentieth century (pp. 221ff.; Knauft 1994b:407ff.).

A further generation of field research will invariably recontextualize works such as *SCNGC.* Known deservedly as a site of rich tribal ethnography, New Guinea has long since entered a world of village-town relations, evangelical transformations, postcolonial politics, and the continuing if compromised pursuit of economic "development." The sites of Melanesian ethnography are expanding increasingly--and appropriately--from the village to include the school, the church, the courts, the disco or cinema, the store, and relatives or *wantoks* in towns or urban centers, as well as the mines, the parliament, and the multinational corporations seeking huge logging or mineral profits.

Melanesian studies now embraces a host of topics that articulate "traditional" concerns with deep postcolonial tensions concerning access to economic development and national resources, fundamentalist Christianity, law and order, government, and identities configured among village, town, regional, and national affiliations. Along the eastern part of the south coast, this last issue engages the problematics of provincial and national identity in Papua New Guinea (cf. Foster n.d.). In the western part, it entails the complexities of accommodation and resistance to massive intrusion by the Indonesian state (e.g., Gietzelt 1988; Monbiot 1989).

Subjectivity and identity become especially complex in postcolonial circumstances of competing or hybrid models of prestige and power. Along most of the New Guinea south coast, for instance, indigenous notions of fertility exchange and sexual power are crosscut by wage labor and personal possession. Even apart from Christianization, notions of interpersonal transaction confront those informed by *bisnis.* Ritual heterosexuality, spouse sharing, or indigenous sexual liaison based on fertility or reciprocity abut Christian morality and competing notions that commoditize sex and create a moral divide between marriage and prostitution or sex-work (e.g., Hammar 1992, 1995, n.d.). So, too, legacies of collective raiding butt against the illegality of *raskol*ism, which is highly developed along parts of the Gulf coast.

Traditions of largess and aggrandizement by leaders abut those of political payoff and postcolonial graft.

The question is not one of "transition" from an indigenous set of values to a Western one but how these conceptions combine to produce new hierarchies and asymmetries of power, stigma, subjectivity, and organization (Knauft n.d.*a*:chs. 4, 6). It is by considering such conflations that what Robert Foster describes as "the New Melanesian Anthropology" becomes both theoretically trenchant and ethnographically current (1995). This project combines an appreciation of local cultural diversity with the ways that actors are linked to regional and ultimately global political economies. There is no need to polarize an internalist cultural perspective against vantage points that stress wider economic and political connection; these dovetail in the study of contemporary Melanesia.

Works such as *SCNGC* provide both historical ballast and ethnographic grist against which to refine such future agendas. Moreover, the work foregrounds how cultural and politicoeconomic forces have always been linked in a dynamic if not dialectical relationship, that is, even prior to colonial influence. These preexisting relationships themselves prefigured the reception, accommodation, and response to foreign intrusion.

Now more than a century old, Melanesian ethnography needs to confront its present through its past. It would be folly to stress the importance of contemporary circumstance without drawing on the strengths as well as illuminating the weaknesses of Melanesia's ethnographic history. The articulation of past concerns to present theory is vital. Correspondingly, serious and critical reexamination of classic information remains one of anthropology's sharp cutting edges. A detailed attention to ethnographic specifics is a key-part of this process: they illuminate and enliven features of gendered, sexual, politicoeconomic, and religious diversity that refract as they twist into the present. Long-standing cultural dispositions are now more important than ever to consider, that is, against the risk of being relegated to obscurity in dusty but still wonderful tomes of classic ethnography.

This edge expands decisively in the light of contemporary ethnographic and theoretical concerns. It can also be reconsidered effectively by Melanesians themselves--not just as informants, but as authors. Only two of the 404 authors referenced in *SCNGC*--Abraham Kuruwaip (1984) and Billai Laba (1975a, 1975b)--are New Guineans.[4] If this represents "the state of the literature," it soberly reminds us of the thickness of the line that continues to separate the authorship of Melanesians from that of Western academics. In addition to issues of theory, method, and content, then, those of emerging authorship--and the enormous diversity of potential authorships

within Melanesia--also engage the limitations and the richness of ethnography's past in relation to the present.

NOTES

1. In particular, the legacy of debate between Obeyesekere (1992) and Sahlins (1995) over Captain Cook is swelling into its own industry. Though Obeyesekere's critique is theoretically scintillating and worthy of exploration in other contexts, it gerrymanders documentary information about Hawaiians and about Cook to a surprising extent; my reading of the data suggests that Sahlins's position is much more factually supported (Knauft 1993).

2. Such versions of what I call "deductive objectivism" have arguably provided the double edge that marks both the contributions and the limits of much French anthropological theory. Spanning from Durkheim and Mauss to Lévi-Strauss and structural Marxism, the benefit gained is large-scale generalization but the cost is relative immunity to refutation or real refiguration on a smaller scale. It has often been the function of empiricism and pragmatism in Anglo-American anthropology to force such refiguration in light of ethnographic counterindications.

3. However, see Hammar's important work on postcolonial sexual practices in Daru (1992, 1995, n.d.).

4. See also Hau'ofa 1975, 1981; Iamo 1992; Iamo and Ketan 1992; Waiko 1992; Kyakas and Wiessner 1992.

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